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of the seed-plants do not appear to fit into this conception in even an approximately satisfactory manner.

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RIVER-BANK MOVEMENTS DUE TO THE EARTH'S ROTATION

TO THE EDITOR OF SCIENCE: In SCIENCE, March 17, Mr. O. E. Jennings calls attention to a difference between the east and west banks of one of the short streams flowing across the almost flat southern slope of Long Island: "An almost imperceptibly sloping eastern bank and a western bank rising quite steeply." Mr. Jennings says, "This peculiar situation has long been accepted rather generally by geologists and physiographers as due to the *westerly deflection* of streams by the earth's rotation" (italics mine). The statement just quoted is doubtless an accidental slip. The fact is that because of the earth's rotation longitudinal rivers in the northern hemisphere erode their right banks—whether they flow south or north.

In offering another hypothesis for those Long Island banks Mr. Jennings makes the justifiable suggestion that the stream in question—as regards length and velocity—is incompetent for securing through the earth's rotation the effects observed. If it has a narrow channel and carries a small volume of water these items should be added to its other disqualifications. And finally, the latitude of Long Island—less than half the distance from the Equator to the North Pole—is none too favorable for river-bank movement due to the earth's behavior as a heavenly body.

In this connection reference may here be made to the unquestionable evidence of the earth's rotation afforded by the Yenisei. There is probably nowhere else in the world any other stream so favorable for the study of bank movement on a vast scale. This for three reasons: This Siberian river is closely longitudinal; of great size; and so far north that a considerable section of it lies within the Arctic Circle. Dr. Fridtjof Nansen, who has sailed up this river from its mouth to Yeniseisk—a distance of more than a thousand miles—writes of the very pronounced contrast between the east and west banks. "Every one going up the Yenisei must be struck with the remarkable difference between the east and west sides of the river. While the flat land on the east is comparatively high and falls abruptly with a steep bank to the river, a steeply sloping beach and relatively deep water outside, the land on the west is strikingly low. The steep river bank is not high, and the bare sandy beach slopes quite gently to the water, with a shelving bottom far beyond it, so that as a rule it is not easy to approach this shore in a ship or boat." And again, "It is striking how much higher and steeper the east bank is than the west everywhere along here."

Dr. Nansen's observations[1] of this northern river and his discussion of what he saw forms a distinct contribution to the literature of the subject of such river-bank movements as are to be referred to the rotation of the earth.

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THE DECOMPOSITION OF TUNGSTEN

SIR ERNEST RUTHERFORD, in the statement copied from *Nature* in the April 21 issue of SCIENCE, was in the very difficult position of being "asked to say a few words" in comment on a brief cablegram to the London *Times* which was itself based on an exaggerated Associated Press dispatch to American newspapers concerning the preliminary and oral but as yet unpublished report of Mr. Clarence E. Irion and myself on the apparent decomposition of tungsten at extremely high temperatures. He mentions the need of a complete report before intelligent comment is possible, but proceeds to make three points which are properly conservative and entirely correct but, as will be seen from the complete paper upon its publication in the *Journal of the American Chemical Society*, which are all irrelevant. In view of the publicity given to Sir Ernest's comments in *Nature* and in SCIENCE, however, a few words in reply are needed.

The first point is that the appearance of helium has often been observed in electrical

[1] Nansen, Fridtjof: *Through Siberia, the Land of the Future*, 71, 72, 73, and 157, 158, etc.

discharge tubes during the past ten years but that "it has been generally assumed that this helium has in some way been occluded in the bombarded material." True; we have a list of no less than 37 papers, most of them published in the years 1912 to 1915, engaged in this inconclusive argument. In spite of the application of the best experimental skill no agreement was reached and Rutherford's conclusion is the general one. Yet there are some of the final experiments, particularly those of Collie, which challenge that conclusion and the problem is still one of the most attractive and important of recent times. Certainly it urges conservatism and the most rigorous criticism, yet not one of the papers shows that helium can not be produced and all call for the application of some entirely new method to the same problem. That we have now accomplished.

The second point is that a measure of the energy produced by the atomic decomposition, as predicted by modern theories of atomic structure, would be "a much more definite and much more delicate test of disintegration of the heavy elements into helium than the spectroscope." This is a rare example of the preference for theory over fact, though saved by the use of the word "test" instead of "proof," and the chemist will be slow to accept it. Our work has not gone far enough to permit the measurement of the energy evolved but the latter is certainly not as large as would be expected from the energy liberated in the disintegration of radium. Yet lack of the theoretical energy does not explain away the formation of a cubic centimeter of permanent gas from half a milligram of tungsten wire, though it demands careful scrutiny and, if confirmed, some explanation. Perhaps a lesser energy content accompanies the greater stability of the permanent metals, for even among the radioactive elements the violence of disintegration varies inversely with the stability.

Finally Sir Ernest points out that no helium has been observed in X-ray tubes operating at 100,000 volts, where electron impacts are even more violent than in our experiments. But the quantity of energy impressed on the target is here minute, the tube current being measured in milliamperes or less, whereas it is the essence of our method to introduce as much as a coulomb of electricity into the wire within 1/300,000th of a second, or many millions of times as much in terms of power. We suppose that it is temperature as such, *i. e.*, the high velocity collisions of the atomic nuclei with one another, that effects the atomic decomposition.

We appreciate and welcome the spirit of Rutherford's criticisms. Indeed it is for the purpose of eliciting such criticism and stimulating the laboratory study by other investigators that we are publishing our work in its present preliminary form. The importance of the problem warrants it.

The real question now raised concerns the broadcasting of the results of scientific researches by our publicity agencies. This is an important function and science has suffered from its neglect. Yet our experience shows that it can be overdone, for here is a research heralded as "transmutation" to millions of newspaper readers in at least six countries: it is not transmutation in any proper meaning of that term, it is merely a preliminary report by no means accepted by, or offered to, the scientific world as conclusive, and it must still wait months before it can be properly published in the appropriate scientific journal for the study of those who are competent to appraise it. Meanwhile it is the duty of scientists to urge prudence and conservative judgment, as Sir Ernest Rutherford has done. Our publicity problems are not solved when we have increased the effectiveness of contact with the press.

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SCIENTIFIC LITERATURE

RECENT WORK ON SOIL ACIDITY AND PLANT DISTRIBUTION

WHEN three independent investigators, living in different countries, and not knowing of one another's activities, hit upon a similar method of study and reach essentially the same conclusions concerning a set of natural phenomena, it is not unreasonable to infer that a correct understanding of the relations has been reached. For many years it has been customary to regard soil acidity as having no particular bearing on the distribution of native